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17th March, 1953.

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WISCONSIN - U.S.A.

Dear Josh,

1 Thanks for the strains and eosin sent with Spicer, and for the list of strains now dried, and for your letters of December 25th and January 12th (and March 11th just received, but not yet digested); my apologies for not writing sooner. My time I found very fully taken up for some weeks with the M.S. which gave me more trouble than I expected: I hope you have one copy, sent off second class air-mail two or three days ago. Could you fill in the information if you have it, at places marked with red ?, i.e. details of H antigens, etc. Also, do you know evidence for calling SW541, SW548 S. t-m?, i.e. were they in vitro O mutants?, and have you report from Edwards of phase 2 antigens of LT2 —X 541, LT2 —X 548, I mean whether they are 1, 2, 3 as I suppose? If you don't have this information I will ask J.T. to look at them, or will try to cadge some anti-2 and anti-3 sera from her; I need the same information anyway about some of my more recent strains. Annoying, but would look ~~well~~ silly just to put them down as 1....

2 The draft as a whole is too long, largely because I have tried to make it as readily comprehensible as possible to non-genetically minded bacteriologists: I have a few paragraphs in mind which I now think I can cut down a bit, no doubt you can point out others. However, I think the J. Gen. Microbiol. will probably not kick even if it is longish. Also as a result of trying to make it easy I now feel I have been a bit over-definite in places; if you can suggest verbal changes to correct this I would be glad. I have tried ~~to~~ it on one or two non-genetically minded people, and it seems understandable, apart from a few ambiguities which I will correct. Am also going to try it on Fraser-Roberts, (the only geneticist in the London School of Hygiene, mainly human) to make sure the bacteriological background is clear enough. I don't know, now, whether it might not be better to cut out mention of the non-lysogenic cells from swarms,

and leave this to go in some later paper, by you or N.Z. on phage: FA relation. I was led to include it because I wanted to mention lysis of induced swarm of SL15 and action of this lysate on SL15 and SL18, but it might be sufficient to say "a lysate was made" and not specify how it came about that this swarm was lysable with PLT22. Furthermore, I think you noticed the "contaminated" growth first, (I think I may have included other things that you did rather than N. or I, but I take it this is OK by you). I am fed up with the sight of the M.S., but by leaving it alone for a few days shall I hope recover enough to re-do it when you and N's comments come in.

3 Phage-type results from Felix.

SL15 group (see draft).

SW543)
609)
613) all react as S. paratyphi B, Vi-phage type "Jersey"
623)
633)
643)

SW546)
534) susceptible to all para B, t-m and even para A typing
588) phages, i.e. "untypable" or "degraded"

(Report on SW703, formerly alleged parent of SW534, not yet to hand).

SW547 S. t-m Vi-phage type 2

SL31)
SL32) para A, Vi-phage ~~provisional~~ type 1.

SL51 S. para B, type 2.

SL54 (not yet reported).

SL55)
SL56) S. t-m, type 2.
SL58)

That is, all as expected.

4 I cannot make out why Felix is so down on Boulgakov: I went into his suggestion that the original phage was specific for d, unlike the one we now have. Actually B's papers show his original phage also attacked para B. etc. F. is also being a bit

hard to convince that his old stocks of 0.901 give H spontaneously; I intend to take up some swarm plates to show him. I don't think he has grasped what a sensitive test semi-solid agar is. Incidentally the old 0.901 stocks do seem a bit more stable than the one you had, or maybe I understand the technique better: anyway I find PLT22/gallinarium 588 gives trails and(probably) swarms on 0.901.

5 I agree we ought to chart Fla loci in some strain sensitive to X phage. None of the t-m I have here are fully sensitive to it, though one can get O's from SW592 and SW544, (H mutants of SW544 and SW549). You may remember I did this at Madison, and found that the slow or fast swarming character of 544 derivatives was unaffected by H to O mutation and then transduction back of H. Incidentally I recently grew PLT22 on SW619 & 620, and put these back on SW544 which gave slow swarms. I am still a bit surprised at the multiplicity of Fla loci; I would be happy if we had one recurrence. If you test many O's in a t-m, I might try the same with a typhi or stanley; typhi would be interesting in view of evidence that gallinarium has at least one Fla gene (am about to test it for a H_1 gene).

6 Origin of para A, O's, SL13 and SL14. These are both in vitro O mutants picked up by Felix from para A, H strains A17698 and A2035 respectively (strains are described in J. Hyg. Dec. 1952). I have no information on whether SL13 has XII_2; I will send it to J.T. who says she has sera which ought to be anti XII_2, but she seems a bit doubtful about this differentiation which she does not normally investigate. K's papers on it seem a bit unclear. I used to think XII was just a symbol for cross-reactivity, like 1 and e, but if one can make a pure reagent to detect XII_2 this cannot be so. K. in his earlier paper talks about an absorbed anti XII_3 serum also, but I see he does not mention it in his book. I gather from Clive S. that Edwards no longer works on XII_2 - XII_3, so I suppose the only way to get it cleared up will be to send SL13 to K. to test. I am not specially concerned with the phage end so will not do this unless I hear that you are not doing so. K. (I think) mentions several para A, O strains of Edwards. These might make good controls if you can get hold of them; also his XII_2 para A strain.

7 My mistake about the dublin → X t-m business. I misread your symbol.

8 Thanks for details of the German papers; my conscience smote me after I wrote to you of them, I went to look them up and was relieved to find both away being bound. Have you any information on effect of temperature on flagellation in S? Jordan et al. 1934, J. Bact. 27-165 had a para B motile at 22°, non-spreading

in passing
at 37° and Gardener mentions/in "Microbes and Ultra-microbes" that at 42° flagella formation is suppressed, but I can't find any detailed work on this. I have re-tested all the O's I have at 23°. SL28 (heidelberg O) spreads slowly at 23°, all the rest remain O. Same temperature-sensitive ones would be useful as a source of doubley-non-motile strains.

9 Have not looked up Andrewes' paper again yet. Am dubious about difference in mutability on agar and in broth as usually impossible to disentangle effect of "accidental selection" which I think likely to be very prominent in agar passage. But I agree there are probably mutations à propos mutability of phase, to account for failure of earlier workers to detect phase 1 in strains in which it is now easy to detect. No satisfactory proof of this, but J.T. etc. all seem to believe it.

10 I don't understand your finding of more double transductions in late swarms, as I have had very few late swarms, i.e. not visible at say 15 hours. Was this in experiments using serum for recipient's latent H antigen? If so, may it not result from motilised cell being di-genic for H antigens at first, and therefore perhaps having two antigens?

11 Have noted your remarks about numbering of Fla loci. I will copy out my chess-board results to date soon, and send it off. There are too many loci. It is specially awkward with strains like SW543 which seem to be bad as either donors or recipients in crosses with t-m. I suppose they must be numbered. It might be best to restrict numbers to strains which are either interesting à propos linkage, or which are efficient as gene donors and acceptors against the most amenable of the present lot of O's. The Paralysis locus in SW573 is different from those of SW578 and SW580 I think.

12 Sorry to have sent you strains without covering letter. I stuck up their numbers on the wall, to wait till I had time to write, paper has now disappeared, but I think they were as follows:

SL28 O strain of Group B. On transduction gives r ⟷ 1...
SL 47 .. .. : appears to be in SL15 group (t-m O)
SL 48 .. .. (.. .. ..)
SL 51 .. .. .. para B. O. (& newer strain)
SL 54 .. .. .. t-m O. not same group as SL15
SL 55, SL 56, SL 58 .. .. .. probably identical. Not .. ..

All same antigen probably

Sorry about these unlabelled strains: if list
& numbers on bottles don't agree, I will
straighten out.

Lots more things to be said but must
wait till a later letter.

I have more or less been offered what
seems like a rather good appointment, & don't
know whether or not to let my name be put up
for it.; this is rather distracting

I have not done final page of MS yet; do
you think it necessary or advisable to re-refer to
difference between transduction & re-combination in K12 here?
Not terribly relevant perhaps.

I saw Bill Hayes at a meeting 2 or 3 weeks back:
he has some nice stuff coming up on effect of anti-
serum on recomb., maybe you have heard of it
already; also Bowley has some nice results on amino-
acid sensitivity in K12 as a marker character,
incidentally he says his strain of the BOT, which
came from Cavalli to us, still has a
demonstrable biotin requirement in presence of
methionine.

Yrs Bruce

Am sending copy of this to
Watson also, (re page MS).